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EXPLANATION THROUGH REPRESENTATION, AND ITS LIMITS

Abstract. Why-questions and how-possibly-questions are two common forms of explanation request. Answers to the former ones require factual assertions, but the latter ones can be answered by displaying a representation of the targeted phenomenon. However, in an extreme case, a representation could come accompanied by the assertion that it displays the only possible way a phenomenon could develop. Using several historical controversies concerning statistical modeling, it is argued that such cases must inevitably involve tacit or explicit empirical assumptions.
Key-words: explanation, representation, synthetic a priori, model, probability, Buffon, Bertrand, Jaynes.

Riassunto: La spiegazione attraverso la rappresentazione, e suoi limiti. Le domande-perché e le domande come-è-possibile sono due forme comuni di richiesta di spiegazione. Le risposte alle prime richiedono asserzioni fattuali, ma alle seconde si può rispondere mostrando una rappresentazione del fenomeno messo sotto esame. Comunque, in un caso estremo, una rappresentazione potrebbe venire accompagnata da un'asserzione che mosti l'unico modo possibile in cui un fenomeno potrebbe svilupparsi. Usando parecchie controversie storiche concernenti la modellizzazione statistica, si argomenta che tali casi devono inevitabilmente implicare assunzioni empiriche tacite o esplicite.
Key-words: spiegazione, rappresentazione, sintetico a priori, modello, probabilità, Buffon, Bertrand, Jaynes.

The typical request for explanation in science, or posed for a scientist, is a *why-question*, that is to say, a request for a missing bit of factual information - the "missing bit of the puzzle". "Why is the sky blue?" is answered by providing information about atmosphere and optical phenomena.

A representation cannot be an answer to a why-question, so it cannot be an explanation in this typical sense, for it cannot provide new information. Only an assertion can do that. The relevant assertion may well concern a representation. For example, the assertion that the representation is accurate

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in certain respects may constitute, or be involved in, the explanation of why certain things happen as they do.

But there is, both in daily life and in the sciences, also another form of request for explanation. A *how-possible question*, unlike a why-question is not a request for factual information about the case, but for an (empirically and mathematically informed) act of imagination.

1. The how-possible question and its limits

Such a request comes in the form of "Show me how this *could possibly* come about" rather than "Tell me why it happens". It requires one to show *how* that phenomenon *could* happen or could come about or could develop in the way it does.

Accordingly, presenting a representation can suffice to answer a *how-possible question.* Different, mutually incompatible representations – specifically, models – can play that role, in principle equally successfully, with different depictions of how the phenomenon comes about.

Subsequently, an answer to the why-question may emerge if one such representation is singled out as the one to be accepted, to be made part of accepted science. On some conceptions of *pure* science, as opposed to its application, that subsequent step is supererogatory. Witness Descartes' famous statement (1644, prop. CCIV) that "touching the things which our senses so not perceive, it is sufficient to explain how they can be", which view he also attributes to Aristotle (2010, A. 7).

As I presented this role, just now, for representations in explanation, there was an implicit reassurance involved that only empirical information can speak to the facts. What a representation provides is content for the imagination, to help understand how things could be, rather than an answer to how they are. But there is at least logically a way to challenge this reassurance. What if the effort to imagine the *how* were to point unavoidably to one single, unique way? In that case we would have to conclude, it seems, that this is not just how things could be, but how they *must* be. That is where the answer to a how-question would transgress on the domain of the *why*, and tell us what is actual after all.

Scientists' dreams of a final theory, a Theory of Everything which demonstrates that, after all, there could not have been any way for the world to be except what it depicts, did not die with Leibniz. But quite apart from this vaunting ambition, there are indeed troubling cases where the very providing of a scientific representation appears to suffice for explaining not just *how the phenomena could be as they are*, but *why they must be precisely thus*.

If there were genuine, not just apparent, examples of such cases, we would have examples of the *synthetic a priori*: explanations of why the phenomena must be as they are, while logically contingent, yet seen to be necessary once a representation is created. I call this a specter - its paradoxicality is highlighted by Kant (1781) in his section on the *Paralogisms* that there can be no inference from *how we necessarily represent something* to *what it is* (cf. Sellars 1974, Powell 1988).

Aside from Kant's clearly apt logical point, I have no general argument that there cannot be a genuine case in which the mere construction of a representation can establish a contingent empirical conclusion. But I will address some cases.

Despite the Cartesian humility mentioned above, the rationalist was apt to find far-reaching conclusions about nature by a priori reasoning. Thus Descartes offers a proof that the vacuum is impossible (1644, prop. XVI-XVIII), and that space is infinite (1644, XXI). Leibniz (1710, sect. 351) adds that in geometry it can be proved that there are at most three mutually orthogonal straight lines at any point, hence space is three-dimensional. Kant rightly objected to this as circular, and sought to find the reason for three-dimensionality in the form of the laws that govern physical force - very cautiously he suggested that three-dimensionality derives from the fact that these relate force to the inverse square of the distance. But later Kant's suggestion too was seen as pointing to an a priori deduction: the effect of a central force will be distributed evenly over all points equally distant from the center, and this is a three-dimensional sphere if and only if the force varies inversely with the square of the distance.

Such examples concerning the foundations of dynamics have been extensively discussed in the literature. Here I will take up, as guide, putatively a priori reasoning about probability and frequency in nature. The most famous historical example is provided by Buffon's needle problem posed in the 18^{th} century and ostensibly solved by a priori reasoning. To begin then, an exposition of that example.

2. Buffon's calculations and predictions

Starting with a simple game, Georges Louis Leclerc, comte de Buffon, went on to analyze a number of types of experiment in which the outcomes are never certain but whose outcome frequencies may be stable. His analyses derived probability functions apparently a priori.

Over the next two centuries there were a whole series of reports that claim remarkable agreement between Buffon's calculated probabilities and the actual frequencies found. But an actual frequency is an empirical fact - how could it possibly be derived a priori?

2.1 Le jeu de franc-carreau, simplified

Buffon started with a simpler problem, the game of *franc-carreau* (1733). In that game a coin is tossed onto a checker or chess board, and bets are placed on whether the coin will land wholly inside one square or 'step on the cracks'.

To simplify the problem even further, let us imagine just long parallel lines drawn on the floor, distance D apart, and a coin of radius R with the diameter 2R definitely smaller than distance D. What is the probability that this coin will land between two lines? In this diagram we see the two extreme positions the coin can have with respect to the lines, and still be inside them:

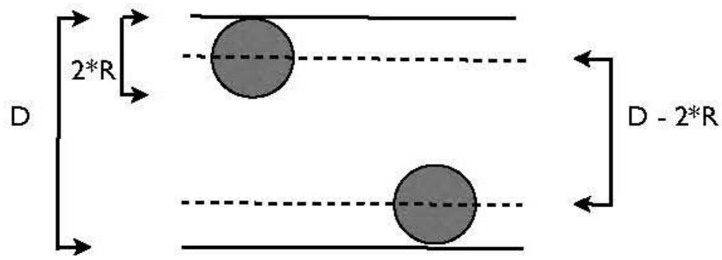

Figure 1. Game of Franc Carreau

The center of the coin has to be at least R from the nearest line, so there is a strip with width D - 2R which is 'favorable': the center of the coin has to be in that strip to be inside the lines. The probability of falling within this, *if the tossing is random*, is (D - 2R)/D and the probability of cutting one of the lines is therefore 2R/D.

Should this probability guide our betting? To say **yes** is to accept the result as an empirical fact about the series of tosses to be carried out, to think that the actual frequency will approach this number 2R/D if we toss often enough.

For Buffon's needle problem, which I will discuss below, there are many computer simulations, today easily accessible on the web. They demonstrate how quickly the actual frequency approaches Buffon's calculated distribution. No doubt a similarly designed simulation for the game of franc-carreau would show the same.

But in this simple example it is quite easy to see the ellipsis in the argument from mathematics to empirical prediction. The missing premise lies here: whether or not the tossing is random is itself an empirical question.

In the above argument, the randomness entered in the form of an assumption about the probabilities involved. It was assumed that equal areas had an equal probability of including the location of the center of the coin. With this assumption, the conclusion does follow by a priori reasoning. To translate it, or extrapolate it, into an empirical conclusion, we need an empirical criterion for random tossing. Such a criterion must not involve a use of the concept of probability, for it is probabilities that we are meant to be finding.

Better not assume something like this: that "random" is defined by stipulating that the tossing is random if and only if the probability of the coin's center landing in any region is proportional to the area. For then we have no empirical prediction but a tautology. Rather, random tossing has to have, as its criterion, a description of the manner of tossing that prevents any systematic bias. Methods that could be suggested are, for example, that the coins are tossed by the handful or by the bucket full, by blindfolded people standing around the edges of the floor, or by raining down coins from a balcony overlooking the floor, etc.

Whatever that criterion is, that it should be represented correctly by a 'flat' probability distribution in the calculation, is in no way to be known a priori.

In fact, we have here a glaring example of what Kant derided as a paralogism: the subject of the major premise is a representation while the minor premise is about what is represented. The conclusion drawn trades on this ambiguity.

For this simplified *jeu de franc-carreau* we can see what has to be assumed, though the assumptions are of great generality. Looking again at the diagram, we see that the situation *as represented* has complete translational symmetry. There is only one probability measure that will respect this symmetry - just the one we found. What is not certain a priori is that any given physical set-up will display this symmetry in its outcomes.

Even if we take for granted that asymmetry can only come from asymmetry (Curie's Principle - surely contestable as well!), the problem remains. For we cannot rule out that there are hidden asymmetries in nature - let alone in games involving people, blindfolded or not.

2.2 Buffon's needle problem and its a priori solution

Buffon's needle problem, though of the same type, is more complex, and the claims of its empirical accuracy more startling. Once again we have a large number of parallel lines are drawn on the floor, but this time a needle is dropped. What is the probability that the needle cuts one of the lines?

To keep the calculation simple, let the lines be exactly two needle lengths apart. Touching will count as cutting, but clearly at most one line is cut. The question is then: what is the probability that the needle cuts the line nearest to the needle's point? (If the point is precisely half-way between, take either line.)

As depicted below, with the needle's length as unit, its point is a distance $0 < d < 1$ away from line L, and its inclination to L is the angle.

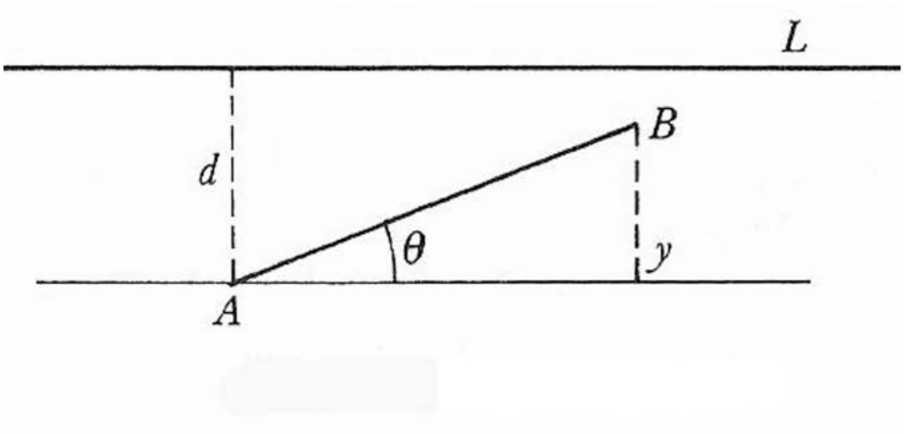

Figure 2. Buffon's needle

The 'favorable cases', that is the cases in which the needle does cut that line, are easily determined: they are those in which $d \leq y = \sin \boldsymbol{\theta}$. Angle $\boldsymbol{\theta}$ varies from zero to $2\boldsymbol{\pi}$ (= 360 degrees), so we can diagram the situation with an area of 1 (needle length) by $2\boldsymbol{\pi}$ (radians) as follows:

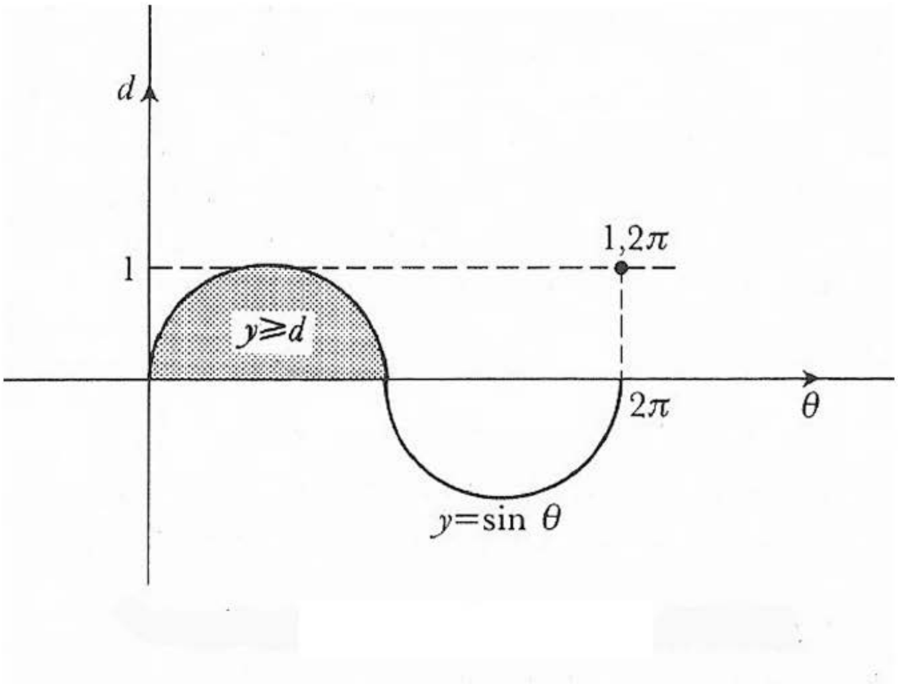

Figure 3. Buffon's probability distribution

To distinguish the favourable cases from the unfavourable ones, we draw in the sine curve and shade the area where $d \leq y$. Assuming independence and uniform distribution, the probability of the favourable cases must be proportional to the shaded area. Although Buffon did not use calculus in his calculations (cf. Holgate, 1981), we can do so and readily demonstrate that the shaded area equals 2. Therefore the probability of a favourable case equals $1/\boldsymbol{\pi}$.

This is just the solution Buffon himself found.

Since the experiment can be carried out, we are dealing with an empirical prediction. The empirical frequency, meant to approach $1/\boldsymbol{\pi}$ can be compared with spatial measurements via the formula for the area or circumference of a circle. As Laplace noted, the experiment can thus – if taken at face value – be used to calculate an ever-increasingly precise value of **π.**

Buffon's needle experiment has been carried out a number of times and reported outcomes have been in excellent agreement with Buffon's prediction. To cite but one example, Augustus De Morgan, the well-known logician, had a student do the experiment with 600 tosses and arrived at value 3.137 (De Morgan 1872, pp. 283-284). He reported another trial with 3204 tosses, arriving at the value 3.1553. They must have been plumb lucky...

For the reports are subject to grave doubts. As Gridgman (1960) pointed out, to estimate **π** to the first decimal, with a 5% confidence interval, would take almost 10, 000 tosses already. To get beyond the familiar approximate value 3.14 would take three years of continuous tossing at the rate of one toss per second**.**

3. Critique of Buffon's needle solution as explanatory

Two representations, one of them Buffon's, can be presented, with different implications for the empirical results. Analysis of the assumptions involved in Buffon's representation, and their relation to how the experiment is set up, clarifies the implicit 'construction' of the phenomenon, and the means by which his model shows how the surprisingly robust empirical results could come about.

3.1 Reconstructing Buffon's needle

From the outset the standard presentation of Buffon's needle ignores marksmanship as irrelevant. In any problem, the division between relevant and irrelevant features marks the symmetries of its representation. In effect, the problem is represented as pertaining to a situation with translational and rotational symmetry, and we will now reflect on how appropriate that is.

We choose a frame of reference in which we take as *X*-axis a line through needle point *A* which is parallel to the drawn lines, as in our first diagram. A is at the origin of this frame, and point B has Y-coordinate *y*. The line *L* has Y-coordinate *d* while angle **θ** is the inclination of line *AB* to the *X*-axis.

In contrast to Buffon, we could begin by assuming that *y* and *d* are independent and uniformly distributed. To do this, we need to describe *y* so that it does not depend on *d*. But *y* is just sine **θ**, and Buffon too took the distributions of d and **θ** to be independent.

The distance *y* ranges from -1 to + 1 (being measured from the *X*-axis, chosen so that the line *L* has equation $Y = 1$). The possible and favourable cases are then depicted in this diagram:

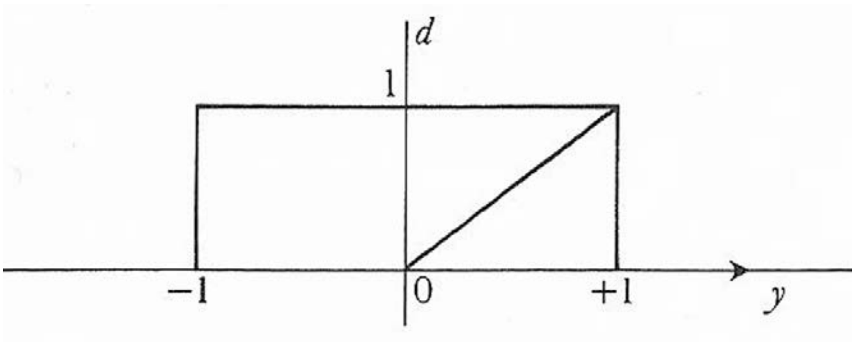

Figure 4. Buffon: alternative calculation

The probability of $y \leq$ d, as depicted here in the triangle to the right of the diagonal line, equals1/4.

Thus by assuming a uniform distribution on coordinate y, we have arrived at a different solution that has nothing to do with the number π. But the reasoning here appears to be exactly similar to Buffon's reasoning, with the problem *differently but equivalently described*.

However, in this reconstruction we have still presented the problem as pertaining to a situation with translational symmetry. But rotational symmetry, a basic assumption in the standard presentation (and in Buffon's own) has gone by the board.

This is where the two solutions, with their different predictions, will differ when the experiment is repeated with varying rotation. Imagine that the axes are rotated through some angle around point *A*; equivalently, that the orientation of the lines drawn on the floor is changed. There is no difference as far as Buffon's solution is concerned. The angle which the needle makes with the *X*-axis is changed by adding something (the angle of rotation), modulo 360°. Rotational symmetry implies that the probability measure has a uniform distribution on the angle, and that is preserved under that transformation: equal angular intervals continue to receive equal probability. But if the probability measure has a uniform distribution on the

Y-coordinate of point B, the calculation changes. If we then transform the situation by rotating the plane, the distribution on the new coordinate *y'* is not uniform. That is apparent immediately upon reflection that, under a rotation, equal increments in *y* do not correspond to equal increments in *y'*.

As Buffon presented the problem it was entirely natural to assume that any adequate representation must observe both translational and rotational symmetry. But why does that feel so natural?

If we imagine someone standing in one corner of the room, throwing needles on the floor with his right hand, there is no warrant for assuming that the distribution of needles will be uniform across the floor and in every direction. We have to imagine instead a uniform *rain* of needles, falling freely along parallel straight lines from above, with uniformly distributed starting points. Whether this could possibly be an accurate representation of any but a celestial game is highly doubtful.

But it will be argued that by not specifying initial throwing conditions, Buffon's problem, rightly understood, pertains to a long run distribution in which the throwers have many different positions and many different throwing tactics. The averaging must then cancel out any translational or rotational asymmetry.

At this point, however, it is clear that the averaging will do that only if the long run sequence of tossing involves the right sort of variation. The assumption that this is so, also assumes a symmetry of sorts, that would constitute the physical correlate of randomness. And while this may be definable, there is no empirical certainty in any manufactured situation that it will characterize what will actually happen in that situation. So we are back with the analytic statement that the prediction will be true if the situation is accurately represented, plus the synthetic, contingent claim that the symmetries in the representation reflect symmetries in the physical situation. No synthetic a priori in sight!

4. Bertrand's paradox - more about a priori explanation

A stick, tossed randomly on a circle, will mark out a chord XY. How long is the chord? More realistically, what is the probability of the various lengths it can have, between 0 and the maximum, which is the circle's diameter?

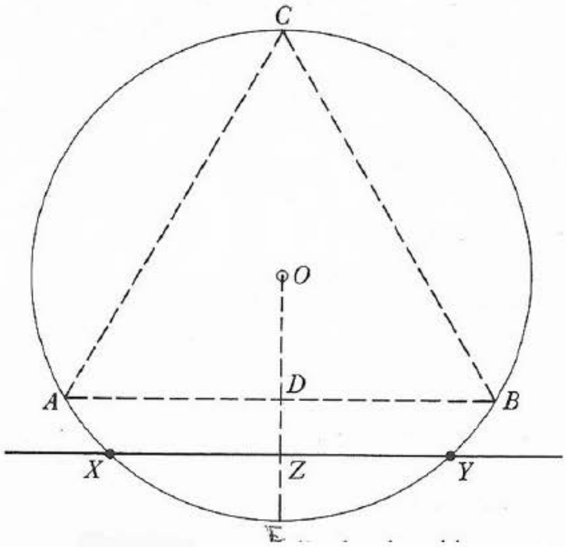

Figure 5. Bertrand's chord problem

The question can be put in a simple form with reference to an inscribed equilateral triangle *ABC* in the circle. However we draw the triangle, it is clear that the separated arcs, like arc *AEB*, must each be 1/3 of the circumference. Thus the length of the side of any such inscribed equilateral triangle is $r\sqrt{3}$, where r is the radius.

The midpoint of a side of the triangle (such as point *D*) is exactly halfway along the radius (*OD* = *DE*). We can thus imagine furthermore an *inner circle* with center *O* and radius $r/2$ that touches the midpoints of the triangle's sides.

So we can make the above question specific as follows:

What is the probability that chord *XY* is greater than side *AB*?

4.1 Bertrand's three solutions

Bertrand could see three different solutions, all at odds with each other - and hence a paradox! The reason is that there are three equivalent conditions posed by this question:

$XY > AB$ exactly if any of the following equivalent conditions holds:

a) $OZ < r/2$ That is: the midpoint of the chord is at an appropriate distance from the center

b) Y is located between 1/3 and 2/3 of the circumference away from X, as measured along the circumference

c) the point Z falls within the 'inner' circle with center O and radius $r/2$.

These conditions specify three different parameters, and for any of the three we can say: well, that is random if the toss is random, so we should postulate a uniform distribution for it. But this gives us three different solutions:

A. (*Solution A*) Using description *a*): OZ can be anything from 0 to r; the interval $[0, r/2]$ of favorable cases has length ½ of the interval $[0, r]$ of possible cases; hence the probability equals 1/2.

B. (*Solution B*) Using (*b*): for each point of contact X, Y can be any point on the circle. So given the point X, we can find point Y at anyfraction between 0 and 1 of the circumference, measuring counter-clockwise. Of these possible locations, one third fall in the favorable interval (1/3, 2/3); hence the probability equals 1/3.

C. (*Solution C*) Using (*c*): the center Z of the stick can fall anywhere in the whole circle. In the favorable cases it falls in the 'inner' circle with radius $r/2$—which has an area ¼ of that of the big circle. Hence the probability equals ¼.

This paradox has been much discussed, and just as in the case of Buffon's coin and needle, it has been claimed that proper attention to the symmetries of the problem determines a unique solution. This was argued both by Henri Poincaré, early on, and much more recently by the physicist E. T. Jaynes, famous for his 'maximum entropy' formalism for probability updating.

4.2 Jaynes on Bertrand's 'well-posed problem'

E.T. Jaynes notes that the three offered solutions lead to mutually contradictory results, but asks (1973, p. 478):

> But do we really believe that it is beyond our power to predict by "pure thought" the result of such a simple experiment ? The point at issue is far more important than merely resolving a geometric puzzle; for (...) applications of probability theory to physical experiments usually lead to problems of just this type

(...) and nothing in the given data seems to tell us which distribution to assume. Yet physicists *have* made definite choices, guided by the principle of indifference, and they *have* led us to correct and nontrivial predictions of viscosity and many other physical phenomena.

Jaynes conjectures then that "in the limit where the skill of the tosser must be described by a 'region of uncertainty' large compared to the circle, the distribution of chord lengths must surely go into one unique function obtainable by 'pure thought' ".

As stated, Bertrand's problem concerns a situation with rotational symmetry, but this does not serve to rule out any of the three solutions. The statement does not specify the relative size of circle and stick, though it must be understood that the stick can mark out any chord, so it must be longer than the diameter. Presumably tosses in which it lands only partially in the circle are simply not counted as relevant. But both points become moot if we simply think of the stick as *selecting* a straight line that crosses the circle, by where it lands, whether or not it is long enough itself or cuts the circle at all.

Leaving angle (orientation) and scale aside then, we must finally look again to translational symmetry. Hence Jaynes imagines a small displacement of the circle, and lays down the condition that in an area inside the overlapping part of the two circles we should see the same distribution. He draws the following diagram (Jaynes 1973, p. 484):

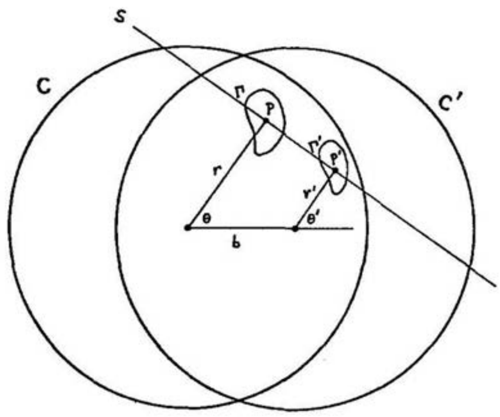

Figure 6. Jaynes' 'well-posed problem'

What exactly is varied if the circle is shifted a bit in one direction or another? The original center plus the displaced center determine a straight line, which is at some angle to the stick. The only difference we can see then between the original and displaced situation is in the distance between center and chord midpoint. If that is irrelevant, is not among the factors that affect the probabilities, then all distances between center and chord midpoint must be equiprobable, so to speak - that is, to be more precise, that the correct probability assignment must imply a uniform distribution over those distances. But that is precisely Solution A. (At this point, the probability density is uniquely determined to be $f(r) = 1/2\pi Rr$, where r is the distance of the chord's center from the circle's center, and R is the circle's radius.)

While there is not such a great lore of amateur experimentalists testing Bertrand's solutions as for Buffon's needle, Jaynes reports: "The Bertrand experiment has, in fact, been performed by the writer and Dr. Charles E. Tyler, tossing broom straws from a standing position onto a 5-in-diameter circle drawn on the floor. Grouping the range of chord lengths into ten categories 128 successful tosses confirmed Eq. (13) with an embarrassingly low value of chi-squared. However, experimental results will no doubt be more convincing if reported by others" (Jaynes 1973, pp. 486-487).

4.3 Friedman's critique of Jaynes

Kenneth Friedman, a critic of Jaynes' 'maximum entropy' program, also offered a quite different approach to Bertrand's chord problem.

Suppose the stick is dropped, with one end at point P. If we assume rotational symmetry, then there will be a flat distribution on the values of angle **θ,** which then determines the probabilities of the lengths of the chords selected in the circle.

This probability function depends on the ratio r/d, of the circle's radius to the distance between its center and the point P.

Will this agree with Jaynes' solution? Not for any given point P, but only in the limit, as P moves farther and farther away, and the ratio r/d approaches zero.

Of course it can be objected now that the distance of point P from the circle is itself a random variable, and we should impose more than rotational invariance. But the main point Friedman wanted to make is that for relatively small ratios for d to r, when the point of the stick is not too far from the circle, there is very little difference between Jaynes's solution and this one. For example, for ratio d/r = 2, the maximum difference between the two predicted distributions is about 6%. So any experiments carried out with less than astronomical numbers of tosses, or tosses separated by a large range of distances for where the outside point lands, is not going to be telling between the two predictions.

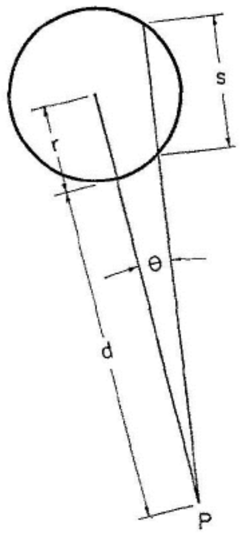

Figure 7. Friedman's alternative calculation

Moreover, and this is a more important point, in any plausible representation of such an experiment we will have a strictly limited range for the ratio d/r. Therefore, once again, whether the apparently a priori deduction should be a guide to our predictions is really a question about the structure of the physical set-up, and about the purely empirical questions of the criteria for the sort of physical invariance that can be adequately represented by a model of a given sort.

To put it in a nutshell: the one problem that cannot be avoided, and that thereby removes the specter of a synthetic a priori created by arguments from inevitabilities in representation, is the problem of coordination.

5. The problem of coordination

The Buffon and Bertrand examples reveal two distinct points that preclude reason from yielding a priori certainties about empirical phenomena.

In both cases, it was clear that there have been no significant empirical tests; only astronomically many tosses, in either case, would yield significant tests of models that all give answers to the *how-possible* question for realistically carried out experiments. But that is a minor point compared to the fact that inferences concerning the phenomena, based on the model, must involve significant empirical assumptions. And these assumptions may be hidden by theoretical descriptions that are subtly circular.

In both cases, the deductions hinge on assumptions about symmetries in the situation, which are empirically contingent.

- As we saw when the experiments are imagined in some fairly concrete fashion, it is in fact not at all easy to arrange for the set-up not to favor some asymmetry in the way the needles, coins, or sticks will land.
- But in addition, even if the tossing procedure is refined so as to remove geometric bias, it is still an empirical question whether that will result in invariance of the relative frequencies under rotation, scale, and translation.

Arguments from symmetry are persuasive for a good reason: symmetry principles can effectively guide modeling, and they identify the most fundamental features of a physical set-up. When they guide the construction of a model, that model represents how the phenomena can develop, and thus answer the question of how they could possibly develop the way they do. But the limits are just there; the explanation of how something is possible cannot be turned into an explanation of why it is actual or why it must be as it is.

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